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MONDAY, JANUARY 10, 1927

WHOLE No. 541

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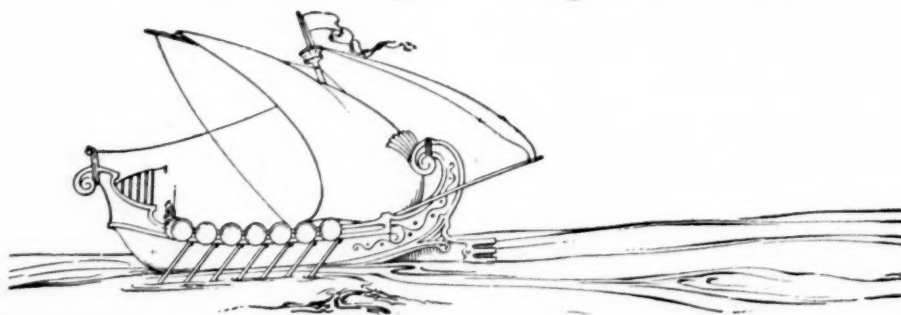
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## THE NEW DISCOVERIES AT MARATHON

About the end of September and the beginning of October, 1926, I made a small experimental excavation in the famous plain of Marathon, precisely where, in September, 490 B. C., the Athenians under Miltiades won their glorious victory over the Persians. A detailed account of my work would be too long. But, since the subject is of interest to every reader of ancient Greek history, I will give a short description of the researches whose nature and results have not yet been made clear by any scholar. They refer to the first, obscure, part of Herodotus's narrative (6.108), that is to say to the identification of the site of the sanctuary of Heracles, around which the Athenians encamped and formed themselves into battle array to meet the advancing enemy. I shall not, however, examine the question from that point of view. I shall not consider where Herodotus's Herakleion was, but I can certify from my own observations that it can not have been in the narrow valley of Avlôna, where many have until now believed that it was situated. No ancient remains or other evidence convinces us that it could have been either there, or at Vraná, a little to the south, beneath the northeast slopes of Mount Agrieliki, as many other writers have thought. For these reasons, and by a reconsideration of the problem, I was able, with the help of indisputable evidence, to fix the position of the ancient city of Marathon. I was convinced that I should thus contribute substantially to the knowledge of the topography of Marathon, and also form a basis for the search for the road followed by the Athenians as they came from Athens to Marathon. I should also definitely fix the site which the Athenians must have chosen for their camp and battle array, for either defence or attack, and, in case of victory, to cut off the advance of the Persians on Athens, which must have been the only reason for their disembarking at Marathon. The question of the site of Herodotus's Herakleion would thus solve itself without need of further evidence.

It is well known where the topographical maps of the plain of Marathon fix three of the four ancient cities, Oinoë, Tricorythos, and Probalinthos. The position of Oinoë is known by the village still bearing the same name. That Tricorythos was near the present Kato-Souli is proved by the many remains of ancient marbles found there. Probalinthos is supposed to have been where Vraná now is. My repeated observations, however, did not justify the supposition that any ancient town had ever existed there. No projecting rock or hill could account, as everywhere else in Greece, for the existence of an acropolis and the consequent ancient town around it; no remains of fortifications are to be seen where they would be expected, if any

town had really existed there, while the marble fragments found by archaeologists in the plain below Vraná could not be from sanctuaries or from the dwellings of men. They are simply relics of funeral monuments or tombs, whose presence there can easily be explained. They are of Roman date, and are not Hellenic columns, but simple pillars without flutings, or bases of little columns: nothing else.

I therefore concluded that one must look elsewhere around the mountain slopes for the probable site of Probalinthos, and I also took into consideration that Probalinthos was not the only one of the group of four cities to be sought, but that the town of Marathon must also be identified. Strange to say, it is marked on no map, nor is it mentioned in any of the works from which the maps were compiled, as for instance those of Finlay, Leake, Ernst Curtius, Kiepert, and Milchhoefer, or in any of the later works of English, German, and French historians. The idea that the town of Marathon could have been built near the bed of the Charadros River, in a low-lying, unfortified position, was short-lived. It is well known to all what positions the ancient Greeks chose for their towns, whether inland or at the sea-side. The place near the Charadros River in no way resembles them.

The names of the three cities, Oinoë, Tricorythos, and Probalinthos, were all derived from their positions in the plain. The word Oinoë means Vineyard; Tricorythos means the Three-Headed Hill; Probalinthos means Projecting-Hill.

Marathon, however, whose name is derived from the word for the herb fennel (*marathos*), gave its name to all the surrounding country. It must, therefore, have been the best-known and most important place in the district, as can be understood by the part it played in mythology.

If I now add that a most likely position for Probalinthos is at the entrance of the roads from Athens and the Mesogaia into the plain, on the left, between the thirty-second and thirty-third kilometer post from Athens, at the foot of the mountain where projecting hills are distinctly seen, then one must expect, and find, an equally likely position in the plain for the city of Marathon, a commanding position which would make it superior to the positions of the other three.

Which, then, would be the most suitable and imposing site? One site alone would justify the building on it of a particularly important ancient town, that is, the part of the plain between the little marsh of Bréxiza and Mount Agrieliki and the narrow pass (100-200 meters) between thirty-four kilometers and thirty-four and a half kilometers from Athens. Here there would be natural fortifications, easy means of communication, closeness to the sea, and, especially, a shore

suitable for the approach of vessels, where even to-day one can clearly distinguish the port built by the Romans (probably in the days of Herodes Atticus), and the pier below the sea, a safe and easy outlook over the plain, and, finally, abundance of spring water (everywhere else only well water could be had).

I think that the above reasons are sufficient to prove the advantages of the position. But I will add that, even if traces of the ancient town were not found, we should nevertheless by conjecture alone—but conjecture well supported—place the site here.

To-day, however, important traces of a town are evident, and we can only wonder why no one has yet taken note of them. There are acropolis walls, two meters broad, made of blocks of stone from the rocky slopes of the mountain on which the acropolis stood. These walls have a circumference of about 300 meters.

They do not, however, extend to the northeast slope of the ridge. This is itself a steep, rocky cliff. The fragments of broken vases which I found while I was excavating belong both to the classical and to the Geometric periods. Some are, perhaps, Mycenaean. One can reach the ridge of the mountain in about twenty minutes from the plain, by an easy path. At the southern foot of the mountain I found a large quantity of flint, a sure indication of prehistoric habitation.

At a short distance from the mountain slopes, where lay the city below and outside the acropolis, the waters collect in large quantities at the edge of the marsh and form the only supply of water, except for the winter rainfalls.

The ancients, or, at least, the Romans, used to convey this water to the sea by a broad canal, which still exists. At the mouth of the canal are the ruins of an ancient Greek temple, and ruins of Roman buildings. Close to the spring mentioned above, near the ruined guard-house on the main road (about 34½ kilometers from Athens) one sees also an ancient aqueduct which, when the excavations start again in the spring of next year, I will examine, together with other remains, more accurately.

We must accept as an undoubted fact that Herodotus's Herakleion was near this town of Marathon, for the following reason. It was of the greatest advantage for the encampment to be as close as possible to an inhabited town, so as to be able to procure direct and immediate help for the army. That is, perhaps, the reason why Herodotus, in his very meager narrative regarding the transport of the Athenian army to Marathon, makes mention of the Herakleion. It will, therefore, be clear to everyone that only by the roads from Athens to Pallene-Mesogaia could the army have marched to Marathon.

I can say that there is no doubt whatever that Probalinthos existed on the spot which I have already mentioned, and where some maps also vaguely place it—at St. John's Metochi (Farm), to the west of the new refugee settlement, called Nea Makri.

The projecting hills (Probalinthos), the spring of drinking water, the ancient Greek marbles from larger

buildings, the excellent arable land, and other signs of ancient attempts to increase the quantity of water will not be described here at greater length.

For the benefit of those interested I wish to add that the well-known stone of Avlóna, bearing the following inscription, 'Ὀμονοίας ἀθανάτου ἀθανάτης πόλη Ἡρώδου ὁ χώρος εἰς ὃν εἰσέρχεται', proved on examination to have another inscription on the reverse side with the following words: 'Ὀμονοίας ἀθανάτου πόλη Ῥηγάλλης ὁ χώρος εἰς ὃν εἰσέρχεται'. The stone came from the arch of a portal, of which the doorposts were of marble, two meters long by .75 meter broad. Blocks of stone and pieces of marble, whitewashed walls .75 meter broad, of Roman date and tolerably well preserved, evidently belonged to some annex of the portal. Two fragments of a huge female statue were also found. Might one of them have belonged to a statue of Regilla herself?

What is still more important, and deserving of careful examination, are the harbor works of the Romans, probably due to Herodes, at the mouth of the canal in the marsh, mentioned above. I hope, when I resume excavations in the spring, to be able to give these more attention.

ATHENS, GREECE

GEORGE SOTIRIADIS

#### TACITUS, ANNALES 2.57

...Postremo paucis familiarium adhibitis sermo coeptus a Caesare <Germanico>, qualem ira et dissimulatio gignit, responsum a Pisone precibus contumacibus, discesseruntque opertis odiis...

A recent rereading of Tacitus has convinced me that in the reading *apertis* in the above passage, found in practically all editions of Tacitus since the days of Gronovius, we have another example of the curious persistence of an idea which has insufficient foundation in fact. The passage refers to the parting of Germanicus and Piso. The indisputable manuscript reading *opertis* was first emended by Lipsius (1607) on the ground that the context necessarily shows that, from the moment of parting, their enmity was no longer concealed, but open. This emendation, so palaeographically tempting, was adopted by Gronovius in his great edition of 1721, and has been followed by Orelli, Anthon, Jacob, Halm, Müller, Allen, Furneaux, Nipperdey, and Fisher.

The manuscript reading has had few defenders. Among these, Salinerius, quoted by Gronovius, saw the importance of the preceding sentence, *sermo...contumacibus*, as an indication that each, as far as possible, concealed his hatred at the time, but involuntarily gave occasional evidence of his real feeling. Brotier (1771), referring to the sentence immediately following, *Post quae rarus in tribunali Caesaris Piso, et, si quando assideret, atrox ac dissentire manifestus*, advances the reasonable theory that Piso would not have appeared on the governor's tribunal at all if the breach at this time had been an open breach. One may well raise the same question with regard to Piso's presence at the banquet in honor of Germanicus, described in the same chapter, and with regard to the statement in 3. 14, ...



*in convivio Germanici, cum super eum Piso discumbet.*...

No one seems to have realized the importance of the words *precibus* and *dissentire*, carefully chosen by the historian, with his usual feeling for the exact word to fit the picture. Piso's veiled insults were in the form of requests, a superb irony under the circumstances. He was still diplomatic enough to appear occasionally (*rarus*) on the tribunal of Germanicus, his chief, and, although he was unable to conceal his real sentiments by his expression of countenance, the Grecism *atrox ac dissentire manifestus* certainly implies that he said nothing. At the following banquet he was so indiscreet as to criticize and to refuse childishly the honors offered to him, basing his refusal on the ground that such oriental luxury was un-Roman. His real reason was that the honors offered to him were inferior to those accorded to Germanicus, though he did not say so. The historian is careful to add *quae Germanico quamquam acerba tolerabantur tamen*. In other words, the fires were still smothered.

In my opinion, the point where the enmity of the two could justifiably be described by the epithet *apertus* was not reached until months afterward (2.62: *Dum ea aestas Germanico plures per provincias transigitur*...). The occasion was the return of Germanicus from Egypt. Tacitus specifically states that it was from this point that there was open and bitter invective between them (2.69: ... *Hinc graves in Pisonem contumeliae, nec minus acerba quae ab illo in Caesarem intentabantur*...). The final step was the formal deathbed *renuntiatio amicitiae* on the part of Germanicus (2.70), made as an affidavit that there had been an open breach, in order that his friends, to whom he was entrusting the case against Piso, might have legal proof of that fact.

It might safely be added that the manuscript reading is quite in line with Tacitus's flair for attributing hidden motives to his characters, as in the case of Tiberius and Domitian. As analogous usages we find in *Annales* 4.67, *occultis... insidiis*, in *Historiae* 2.30 *condito odio*.

WOOSTER COLLEGE,  
WOOSTER, OHIO

FRANK HEWITT COWLES

#### A Parallel to Tacitus, Agricola 18

In the New York Times Book Review, June 13, 1926, under the title *What the Fuggers Knew of Queen Elizabeth*, Mr. Rupert Layard reviewed a work whose title was given as follows:

*THE FUGGER NEWS-LETTERS. Second Series. Being a further selection from the Fugger papers specially referring to Queen Elizabeth and matters relating to England during the years 1568-1605, here published for the first time. Edited by Victor von Klarwill. Translated by L. S. R. Byrne. With forty-six illustrations. 353 pp. New York: G. P. Putnam's Sons.*

The final paragraphs of the review read as follows:

The Cologne correspondent seems a rather credulous young man. On Sept. 15, 1586, he writes of the Earl of Leicester's army, which comprised 1,500 Irishmen: "These Irishmen are almost all naked and have their bows and arrows with them. They are very

quick runners, and there are also some hundreds of them who go on stilts the height of a man. They are to walk through the moats surrounding the towns and climb the walls".

This seems another injustice to a noble race. It must have been painful enough to fight "almost naked", but <to fight> "on stilts" the height of a man ... seems too much to ask even from a Sinn Feiner. We know that armor was plentiful enough in that century, and the explanation of this ostentatious nudity escapes us. As usual we fly to Kipling and wonder if these news-letters could have been a source book for the story of the "Taking of Lungtungpen". We like to think Mulvaney's ancestors took part in this scrap, and we bow to them reverently across the centuries.

While of course an Irish source for this or any other exploit of Mulvaney offers its attractions, yet, if one must suppose that Kipling needed any source-book other than his own imagination or the traditions of the British Army, why not go even farther back, to Tacitus? Stalky and Co. were well drilled in Latin, and readers of Kipling and of Tacitus have long quoted Lungtungpen as a parallel for the vivid, if brief account, in *Agricola* 18, of the taking of Mona.

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MARY JOHNSTON

#### REDEEMING THE FIRST-BORN

In her interesting remarks, *Light on the Aeneid*, THE CLASSICAL WEEKLY 20.43, Professor E. Adelaide Hahn evidently has been misled by the erroneous statement of M. Pallary, as quoted in her article, from Professor Kelsey's book. M. Pallary speaks of "a member of the Cohen family" <the italics are mine>, and is thinking, evidently, of these people as a certain family group. In reality, this custom, known as *Pidyan Haben*, is not limited to the Jews of North Africa, but is the legal prescription among all orthodox Jews throughout the world. I myself witnessed it repeatedly in my youth in Hamburg, and I doubt not that it is observed even to-day, among the orthodox, right in our midst. The duty to pay a small sum of money to a Cohen, that is, to a member of the priestly caste, is based on Exodus 13.2: "Sanctify unto me all the firstborn, whatsoever openeth the womb among the children of Israel, both of man and of beast: it is mine". Aetiologically, the duty is connected with the slaying of the firstborn of the Egyptians during the Passover Night. For the same alleged reason, the firstborn male of every Jewish family, except, I think, the *Coganim*, observes the day preceding Passover as a fast-day. The true explanation has been known for a long time, and has been stated so well by Sir James G. Frazer (*The Dying God*, 178-179; *Folk Lore in the Old Testament* 1. 480-481) that I cannot do better than quote his words (*Folk Lore*, etc.): "The practice of killing the first-born of a family seems to have been widespread, and there are some grounds for thinking that it is based on a theory of transmigration. The soul of a parent or grandparent is supposed to be born again in the person of child or grandchild. But if the person whose soul is thus born again should chance to be still alive at the time of the rebirth, it seems clear that he runs a risk of dying in his own person at the same moment that he comes to life again in his child or grandchild; such a risk may be obviated either by preventing the child from being born, or by killing it immediately after birth".

I am afraid that Miss Hahn's second quotation, regarding the Indian who buried his child alive with its dead mother, has nothing to do with the passage

from Aeneid 8 which she thinks connected with it. In reading the different reports collected by her, we notice that here, too, the child was a male child, and the firstborn—the dead woman was a “bride”. Miss Hahn’s last quotation states expressly that the man “obeyed—because disobedience might have meant death”. There seems here to be revealed a curious confusion in the man’s mind. The child originally had to be killed because it represented the soul of the father. But what was the reason for wrapping it in a blanket with the dead mother and permitting dead mother and living child to lie on the Ute camp ground throughout a February night? According to one of the Associated Press dispatches, “the father believed it would bring her <the mother> back to life”. This seems, indeed, to be the true explanation. Frazer, in the chapter from which I quoted above, has collected numerous examples to show the existence of a belief that the spirit of a dead infant enters its mother’s womb again and is born anew at the next confinement. The belief that it is possible to catch the departing life for another birth is widespread, as is well known, and underlies also the Roman custom of *extremum halitum ore legere*. It is, then, quite possible that the two ideas were contaminated in the Indian father’s mind and led him to commit what appears to the white man as a horrible and cruel crime.

But all this furnishes no explanation of the treatment resorted to in the Aeneid. Whether this is to be traced, with Nettleship, to the historical barbarities of the Etruscan pirates, I do not know. Nor do I know of any parallels from the folklore of primitive peoples, though some ethnologist may know of such parallels. If Miss Hahn’s suspicion should prove correct, the practice may possibly be connected with a belief similar to the one underlying the killing of the babe. However, Vergil conceived it evidently as a punishment. The only parallel which I can quote I read some years ago in a novel, by Jack London, I believe—the title has escaped my memory—where a man pursues his wife’s seducer into the desert, shackles him to himself, and starts back to civilization. They lose their way, and the book closes when one of them has just died of thirst and the other man finds that he cannot slip the bonds and thus is compelled to wait for death, fettered to the corpse of his enemy.

HUNTER COLLEGE

ERNST RIESS

### REVIEWS

New Chapters in Greek Art. By Percy Gardner. New York: Oxford University Press, American Branch (1926). Pp. xvi + 367. 31 Illustrations, 16 Plates. \$7.00.

The bibliography of Professor Percy Gardner’s archaeological writings, appended to his recent volume, *New Chapters in Greek Art* (356–362), reminds us how much we are indebted to him for his essays on numismatics, sculpture, and classical archaeology these past fifty-five years. Conservative in his point of view, he has always been eager to fight vigorously for a position which he thinks amply justified. A stalwart archaeologist and a patient and devoted teacher, he deserves the leisure from academic duties which he now enjoys as Emeritus Professor at Oxford.

The volume under review contains various papers, most of which have been published since 1888 in the *Journal of Hellenic Studies*, the *Journal of Roman Studies*, and in *Corolla Numismatica*. The contents of the volume are as follows:

Preface (vii–xii); Fifty Years of Progress in Classical Archaeology (1–25); Originals, Ancient Copies, and Modern Restorations (26–45); A Female Figure in the Style of Pheidias (46–83); A Bronze Head of the School of Polycleitus (84–98); A Head of Apollo Probably by Scopas (99–116); The Agias of Lysippus and its Congeners (117–162); Cleobis and Biton at Delphi (163–170); Themistocles at Magnesia (171–181); Artemis Laphria at Patrae (182–187); The Delphic Charioteer and the Spinario (188–207); The Athena and Marsyas of Myron (208–215); The *Antioch* of Eutychides (216–268); Greek Art under Roman Rule (269–305); The Scenery of the Greek Stage (306–345); Appendix I: Address at the First International Congress of Archaeologists at Athens, 1905 (346–348); Appendix II: Address to the Society for the Promotion of Hellenic Studies, on Retiring from the Presidency of the Society in 1911 (348–355); Appendix III: List of the Archaeological Books and Papers Published by the Author (356–362); Index (363–367).

The papers fall under four heads. Most of them are technical discussions of sculpture, a few deal chiefly with historical interpretation, one treats of the Greek stage, a subject somewhat far afield from the author’s main interest, and some deal with a defense of classical study.

As a critic of sculpture, Professor Gardner is primarily interested in assigning objects to the proper period, region, and school. With conspicuous caution he musters his evidence, relying largely upon numismatic and literary data; lacking the imaginative insight of Furtwängler, he avoids the criticism which brilliant hypotheses often invite. He also lacks the aesthetic appreciation which is so refreshing and stimulating in French archaeologists, notably Collignon, Perrot, and Pottier. This is unfortunate, for sculpture is, after all, art as well as archaeology. Professor Gardner seems a bit shame-faced before beauty. In writing of the Delphic Charioteer, he calls attention to the fact that a wrist-bone is out of place (190), but foregoes appreciative comment like that of Bourguet in *Les Ruines de Delphes*. He shuns artistic controversy (290), and in his few ventures in appraisal confines himself to comment like this: “I confess to being under the charm which in attitude and drapery this statue so conspicuously possesses”. In other words, he is interested in attribution rather than in evaluation.

Of Professor Gardner’s essays on sculpture the most thoughtfully reasoned and persuasively expressed is that on Originals, Ancient Copies, and Modern Restorations. Here he insists upon the value of originals, and especially those of the Hellenic period, rather than upon prehistoric remains, which have received disproportionate attention from archaeologists of recent years; he notes that copies of the Hellenistic age have more value as art, but that those of the Roman times, being more mechanically correct, are more reliable as evidence for lost originals. This is doubtless true; but it should be noted that such Roman copies must under no circumstances be taken as a basis for the aesthetic evaluation of Greek originals. The galleries of the Vatican are a cemetery of Greek art; and it is devoutly to be wished that every visitor to Rome could

be warned against them and sent instead to the more living classical art at the Terme Museum.

Professor Gardner does an able piece of work in his paper on the Pheidias Figure in the Ashmolean (46-83), which, he suggests, may be a portrait of Aspasia in the guise of a goddess, a statue from the workshop of Pheidias, and also in his study of the Ashmolean bronze fragment of a head, which is Polycleitan (84-98). The head is charming, although hardly worthy of the superlative praise given to it by the author, who supports his contention that it "greatly surpasses" (88) the Idolino and Beneventum heads by citing only the cartilage of the ear (88) and the "star-fish" hair (89-90)! His own aesthetic limitations are revealed when he analyzes "the most remarkable and fascinating beauty" (97) of the head entirely in terms of naturalistic features.

In the course of his study of an Apollo head from the Mausoleum (99-116), Professor Gardner comments sharply on the delay of the British Museum in publishing this monument (99). He mentions Oldfield's restoration (100), but curiously fails to speak of the restoration by Professor W. B. Dinsmoor, of Columbia University.

The most provocative paper in the book is that on the Agias. In this Professor Gardner upholds the thesis that the Agias is Lysippic and the Apoxyomenos is not. Arguing from naturalistic details in the feet and the thighs, the academic quality and the build and the composition of the Apoxyomenos, he concludes that it must be a copy of a work done after the period of anatomical research, about 300 B. C. Considering the Agias as representative of Lysippus's mature genius, he dates the sculptor earlier than the generally accepted period, placing Collignon's dates (350-300) back about twenty years. He also argues that Lysippus was an idealist. Although Professor Gardner has here done a painstaking piece of analysis, I regard this chapter as the least convincing in the book. Space does not permit a detailed criticism of the argument, but it may be advisable to summarize the case against his position. (1) The naturalistic data. The opinion that Lysippus was an idealist is based upon very shadowy evidence. The author writes (140-141),

...I am disposed to think, that under the Plinian phrase, *quales viderentur esse*, there lurks some Greek expression to the effect that Lysippus also was an idealist. The words *οἷους εἶκεν εἶναι* have been suggested as the phrase which Pliny has misrendered, and the suggestion is at all events ingenious....

But the traditional view, that he was a realist, is clearly indicated by the statements of Pliny (N. H. 34. 61) that Lysippus was inspired by a saying of Eupompos, 'Nature herself and no artist was the true model', that he 'represented men as they appeared to the eye', and that 'the extreme delicacy of his work, even in the smallest details, would seem to be its most individual feature'. (2) The academic quality. This, as Professor Gardner admits, it is reasonable to expect in a late copy. (3) The composition and the build. Here, it seems to me, Professor Gardner makes his chief error of judgment. The Agias is a conventional statue;

the Apoxyomenos, in its three dimensional conception and in its rhythmic proportions, is a superb innovation. If the Agias is representative of Lysippus, we may at least declare that he was by no means so daring an innovator and so consummate an artist as the creator of the Apoxyomenos was. But the evidence of ancient testimony is that Lysippus was a man of original genius. As Pliny says (N. H. 34.61), 'He exchanged the squarely-built figure of the older artists for a new and untried system'. The most generally accepted theory, that the Agias is from a work of Lysippus's earlier career, and the Apoxyomenos a copy of one of his mature masterpieces, seems much more plausible.

Another provocative, but more convincing, paper, in many respects the best in the book, is that on Greek Art under Roman Rule. This has to do almost entirely with sculpture; in architecture Professor Gardner would be compelled to grant more to Roman creative power than he does. He criticizes justly Wickoff's theory that the Romans were innovators in sculpture. The Romans did demand a literal recording of fact in their portraits, but neither in "the style of illusion" (a vague term) nor in continuous narration in relief were they original. The theory of Strzygowski (302-304), that Rome was largely affected by oriental influences, and that of Courbaud (303-304), who sees the strongest effect produced by Pergamon and Alexandria, are approved by the author.

It is as an interpreter of the relation of art to history that Professor Gardner, in my opinion, is most successful. In his chapter on Cleobis and Biton, he shows delightfully how Herodotus's stories are substantiated by archaeological finds; in his valuable study of "The Antioch of Eutychides", he reviews the whole question of the personification of city ideals in art. The following quotation will serve to show his deftness in revealing historical relevance in archaeological data. After describing a coin of Magnesia, struck by Themistocles, he says (171):

...there are several curious features in this coin. In the first place, it is unique in its period in Asia as bearing the name of a ruler or despot, a rare privilege granted by the Persian king, showing in what favor Themistocles was held. In the second place, among the known examples a high proportion are not of solid silver, but plated. If these plated coins were issued from the mint, it casts a sinister light on the honesty of Themistocles. In the third place, the money follows the Attic standard of weight, which is most unusual in Asia at the time, and which seems to show that Themistocles wished, even in exile, to keep in touch with Attic commerce....

He gives an acute analysis (172-176) of the falsity of the story accepted by Plutarch and Diodorus, that Themistocles committed suicide by drinking bull's blood.

In dealing with the Greek theater, Professor Gardner accepts the theory of the raised stage (317-322), which, most archaeologists will agree, has been adequately refuted. He believes that Agatharchus introduced architectural perspective on the "backdrops" which he painted for Aeschylus (326-330, 332-334), and that the *periacti* gave short-hand suggestions of scenes (336-340).



As an advocate of the importance of classical study, and especially of archaeology as an essential element in such study, Professor Gardiner writes with much sound sense. He summarizes the recent progress in classical archaeology, praising especially the work of Furtwängler at Aegina as the high-water mark in excavation and description. He sees in these days the urgent need of classical standards; and, although he is unwise in speaking of "the chaos of the impressionists" and "the absurdities of the post-impressionists", thereby revealing again his aesthetic limitations, he advocates with sturdy eloquence the value of classical art in a liberal education.

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*Olympia: Its History and Remains.* By E. Norman Gardiner. New York: Oxford University Press, American Branch (1925). Pp. xx + 316. 129 Illustrations. \$16.75.

With the assistance of Professor W. W. Hyde's *Olympic Victor Monuments* (see *THE CLASSICAL WEEKLY* 17.59-62) Mr. E. Norman Gardiner's *Greek Athletic Sports and Festivals* (see *THE CLASSICAL WEEKLY* 4.100-102), and Mr. Gardiner's present volume on Olympia, the English-speaking student of antiquity should be able to reconstruct for himself in fair measure that ancient shrine and festival which figured so prominently in the social, physical, and spiritual experiences of the Greek peoples. Dr. Gardiner's most recent work is as unassuming quarto volume, well illustrated, and composed in a sober and prosaic, but not unreadable, style.

The contents of the book are as follows:

I. The Destruction and Recovery of Olympia (1-12); II. The Geography of the North-West Peloponnese (13-25); III. The Prehistoric Remains of the North-West Peloponnese (26-39); IV. Peoples and Cults of the North-West Peloponnese (40-57); V. The Origin of the Olympic Festival (58-78); VI. Olympia and Pisa (77-103); VII. Olympia and Elis (104-127); VIII. Olympia and Macedon (128-151); IX. Olympia and Rome (152-174); X. The Topography of the Altis (175-192); XI. The Altars and Hero Shrines of Olympia (193-205); XII. The Heraion (206-216); XIII. The Treasuries (217-233); XIV. The Temple of Zeus (234-266); XV. The Official Buildings of Olympia (267-283); XVI. The Athletic Buildings (284-293); XVII. The Exedra and the Water Supply (294-299); XVIII. The Olympic Festival (300-311); Index (312-316).

Dr. Gardiner is decidedly an historian rather than an archaeologist; hence the historical and the topographical sections of the book considerably surpass the archaeological alike in bulk and in merit. In the opening chapter Dr. Gardiner presents an interesting sketch of Olympia during the Middle Ages, and briefly traces the course of modern exploration and discoveries on the site down to the time when, a half century ago, Ernst Curtius and his colleagues, supported by the German Government, began their monumental series of excavations, which covered a period of six years.

Fully half of the book is occupied with an historical examination of Olympia and her surroundings during

that long period which stretches from the era of the prehistoric village, of which almost nothing is known, until the year 385 A. D., with which is to be associated the name of the last recorded Olympic victor, a boxer, Varastod or Varazdates by name, a Persian Arsacid, from Armenia. Nine years later the games were abolished by a decree of the Emperor Theodosius: the nineteenth century saw their revival. Except as regards matters of detail, the author has perhaps added little to what is already known—apart from a new and convincing theory of the origin of the Olympic festival (63-76), an elaboration of which he had previously published elsewhere (in the *Annual of the British School at Athens* 22). A very similar conclusion was reached independently by H. J. Rose (*Aberystwyth Studies*, 3. 1 ff.). This involves the belief that the festival and the games at Olympia were of entirely different origins. The festival, which may very likely antedate the Greek occupation, was apparently a purificatory ceremonial held at the end of harvest. The games proper, which were introduced by the northern invaders, became simply a "mimicry of noble war", participated in by the most vigorous of the fighting men, and presided over by the war-god Zeus, in a temporary aspect of truce-enforcer. The festival, Dr. Gardiner thinks, was perhaps originally held at intervals of eight years, and he hazards the conjecture that the traditional date (776 B. C.) may recall the year when a change was negotiated from the eight-year to the four-year Olympiad. One of the most interesting of his observations is his assertion (151) that during the Hellenistic period Olympia's chief service to humanity consisted in fostering the tradition of athletic honor. This spirit, indeed, is undoubtedly one of the finest of the social legacies that the ancient world has bequeathed to us.

The sections of the book which treat of questions of archaeology and art-criticism suffer, to a certain extent, by comparison with the work of F. Poulsen on Delphi, a work of very similar purpose and design (see *THE CLASSICAL WEEKLY* 15.45-48). In the present volume we miss that fine critical touch and skilled art of discrimination which distinguish all the writings of the Classical Curator of the Ny Carlsberg Glyptothek. Did we not possess Poulsen's book on Delphi, we should undoubtedly be inclined to rate Dr. Gardiner's work, in this department of criticism, considerably higher. In fairness to the latter it must of course be recognized that his was the more difficult task, for the science of excavation was in little more than its infancy in the days of Curtius, while it had made enormous progress before the French undertook to unearth Delphi. But one may wish, at any rate, that Dr. Gardiner had taken time to revise his chapter on the Heraion in the light of what has been revealed by Dörpfeld's recent reexamination of its foundations. He has shown that three temples were erected successively on the site. He has also proved that the Great Altar of Zeus is a prehistoric house—a fact which the original excavators might have learned for themselves, had it not been for their supreme contempt for potsherds and such 'small objects'. This information



was, of course, available for use several years ago. It would likewise have added to the interest and the value of his book, had Dr. Gardiner seen fit to rewrite his chapter on the sculptures of the Temple of Zeus after the appearance of Schrader's *Phidias*. He merely mentions Schrader's "revolutionary theories", in a note. It may very probably be true that, as Dr. Gardiner maintains, Alcmenes and Paeonius had no part or lot in the matter, and that the pedimental sculptures of the temple are the product of a local school, while the three figures in Pentelic marble on the western pediment represent nothing more than pieces restored in the first century B. C. Certainly the presence of a Pentelic marble arm on a fourth figure goes far to weaken Schrader's theory. But, none the less, no effort ought to have been spared on the part of the author of so important a work to bring his accumulation of fact and theory up to date.

The book shows its fair share of printer's errors, and there are noticeable a few inaccuracies of reference and statement, such as the giving, on page 285, of 200, in place of 600, Olympic feet as the length of the course in the Stadion at Olympia. Furthermore, Professor Hyde has clearly demonstrated (*Olympic Victor Monuments*, 167-171) that the swollen ear is not essentially a distinguishing feature of the boxer, wrestler, or pancratiast. The figure of the Lapith boy in the right end of the western pediment of the temple of Zeus shows this slight disfiguration. The statement of Dr. Gardiner (253)—applied to a member of a purely mythical race—that the lad is therefore a pancratiast partakes both of false and ludicrous elements. It is difficult also to accept a minor feature of his interpretation of the scene on the metope which represents the cleansing of the Augean stables. Heracles, he says (265), "pushes with all his might at the pile of dung before him" with a broom. But why is it necessary to disregard the testimony of Apollodorus (2.5.5), that the hero "made a breach in the foundations of the cattle yard, and then, diverting the courses of the Alpheus and Peneus... he turned them into the yard?" A very similar account of the exploit is given by Pausanias (5.1.9). Surely, then, the relief shows Heracles in the act of cutting his trench.

The last chapter of the book describes the procedure of the ceremonials of the festival itself—perhaps the finest summary of the celebration that has ever been written.

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The Latin Conditional Sentence. By H. C. Nutting. University of California Publications in Classical Philology 8. 1-185. Berkeley: University of California Press (1925).

In a monograph entitled *The Latin Conditional Sentence* Professor H. C. Nutting, of the University of California, has brought together the results of his studies on the conditional sentence extending over a period of twenty years. Much of the material here presented had been published in separate papers.

The chief topics discussed are the following:

(1) Forms of Conditional Speaking (5-34), (2) The Modes of Conditional Thought (45-59), (3) Special Uses of Conditional Particles (60-80), (4) Subjunctive Protasis with Indicative "Apodosis" (81-121), (5) The Contrary to Fact Construction (122-163), (6) Conditional Clauses of Comparison (164-175).

There are also a Register of Passages Cited (176-181), and an Index (182-185).<sup>1</sup>

Professor Nutting has, in this work, adopted an independent point of view and his whole study is thoroughly original. Because of this we should expect to find, as we do find, that the conclusions reached sometimes furnish important contributions to knowledge and are always stimulating to thought. Some of the conclusions, however, can scarcely escape adverse criticism at the hands of those whose syntactical creed differs from that of Professor Nutting. It seems worth while to attempt to point out some of the possible adverse criticisms.

It is well to note that a condition may be represented by an adverb, an adverbial phrase, a noun, a pronoun, an infinitive, an adjective, a participle, or an ablative absolute (15-24). But there is no "condition condensed" in such cases. The idea of condition is simply an implication. Considering the matter thus, we may say that it is a serious defect to omit mention, as Professor Nutting does, of relative clauses, for in some relative clauses there is this same implication of condition.

So, too, in the consideration of what Professor Nutting calls *The Modes of Conditional Thought*, we have to do with implications. In the four modes enumerated—causal (46-49), circumstantial (49-51), inferential (51-55), and predicating (56-59); the acts or the states of the two clauses are in harmony, and there is in each case an implication of some variety of causal nexus. But the acts or the states may be in opposition, and then there will be an implication of an adversative or a concessive relation. But why, in this latter case any more than in the former, should Professor Nutting speak of a special use of the conditional particle (60-67)?

These are minor matters. It is in his treatment of questions having to do with the subjunctive that Professor Nutting will meet with most pronounced dissent. Many scholars believe that the subjunctive modal form meant something definite, while Professor Nutting belongs to the school of Bergaigne and Morris, whose theory concerning the subjunctive might be called a nebular hypothesis. So Professor Nutting hardly allows himself to speak of the meaning of the subjunctive, but speaks rather of its functions. And so he can say (2), "Volition, concession, entreaty and condition are all *coördinate* functions of the same phrase..." This is to confuse two distinct linguistic categories—the expression of modal ideas and the expression of relational ideas.

Professor Nutting, however, is quite right in rejecting (5) a volitive or optative origin of the subjunctive

<sup>1</sup>I note that the Index does not include the names of scholars whose views are discussed, with approval or disapproval—a very serious omission. C. K.

conditional clause. He is quite right in insisting (5) that such collations as *impetum faciat: digne accipietur* have no bearing on the question of origin. But volitive and optative do not exhaust the possibilities for the modal meanings of a subjunctive, and to assign to the subjunctive in a paratactic condition a conditional meaning or function is to fall into the confusion already noted. The subjunctive in conditional clauses as we have them surely expresses something, and that something is not the idea of condition; that is expressed by the conditional particle. Sound method raises the question whether this meaning is not one expressed by subjunctives outside of conditional clauses. Finding an affirmative answer to this question, we should have a clear indication of the meaning of the subjunctive in the paratactic *si*-clause. Now all this sort of thing seems to be quite outside Professor Nutting's plan of investigation, and the worst of it is that the virtual denial of modal meaning for the subjunctive in conditional clauses tends to becloud the discussion of all problems of the subjunctive condition. How can one discuss (81) the "inconcinnity" in sentences with subjunctive protasis and indicative apodosis, such as Plautus, *Miles Gloriosus* 571 *Ne tu hercle, si te di ament, linguam comprimēs*, without first making clear just what *ament* means? And one can hardly hope to get a clear understanding of conditional sentences of this sort which have a 'modal' verb (*possum, debeo*, etc.) in the conclusion, until he has formed a definite conception of the relations existing between modal ideas, verbal ideas, and tense ideas.

More difficult of acceptance still is Professor Nutting's treatment of the "contrary to fact" condition. In the first place he will have it that in early Latin the *si*-clause with present subjunctive quite commonly expresses a present contrary to fact condition (122). In this he is in agreement with Heinrich Blase (*Studien und Kritiken zur Lateinischen Syntax*, II Teil, 23 [Programm, Mainz, 1905]). Now the idea of contrariety to fact is an implication, as Professor Nutting himself incidentally notes (122). In the case of the present subjunctive certainly neither the tense nor the mode does or can make the implication. Professor Nutting strikes close to the truth when he says (125),

It is true that the present subjunctive was the standard expression also for the condition of the vague future type<sup>2</sup>. But with the help afforded by circumstance, context, and (in oral delivery) by intonation, the hearer was not left in doubt in cases where the contrary to fact idea was to be conveyed....

The truth is that the factors mentioned give the implication, just as they do sometimes with present subjunctive in later Latin. So, too, the imperfect subjunctive in a condition is innocent of any implication of contrariety to the fact of past time.

Further, Professor Nutting seems to be at one with Blase in thinking that the imperfect subjunctive in conditions contrary to fact in the present really loses its preterite meaning and becomes a present (133). On this topic Rudolph Methner (*Lateinische Syntax des Verbums*, 177 [Berlin, 1914]) has said about all that

<sup>2</sup>One is inclined to become impatient with the use of the term "vague future", though this is only a little worse than "less vivid future". How can a future any more than a present or a past be vague?

needs to be said: "Nur wegen dieser präteritalen Bedeutung kann er der Begriff der Irrealität ausdrücken". Surely *fuit* in *Troia fuit* does not become present tense because of an implication that Troy is not now. When Horace makes Trebatius say (*Serm.* 2.1.16) *at tamen et iustum poteras et scribere fortem*, the imperfect *poteras* keeps its preterite meaning notwithstanding the implication that the person addressed is *not* writing on the subject indicated.

For Professor Nutting the main problem of the "conditional clause of comparison" has to do with the contrary to fact implication commonly present (164), though "the great majority of cases accord with the law of sequence". But he himself argues that the implication in the great mass of clauses with *quasi* (*tamquam, velut*, etc.) is something different from the ordinary contrary to fact implication, and, while we may not agree with his argument, we should agree that no implication of contrariety to fact is given by the form of the verb. Unfortunately his methods do not allow him to start with the determination of the true force of the subjunctive in these clauses and the true character of the conditional clause itself. He does show, however, that in some cases the *si*-clause is not future and that, in consequence, the conditions can not be classed as "vague future". But "vague future" condition is a man of straw. Here, as for the most part elsewhere, the condition with the subjunctive is an ideal condition. Since now the condition is expressed as ideal and not as real, the way is open for an implication by the context of contrariety to fact. In some sentences quoted, for example Cicero, *Tusc. Disp.* 1.86 (165), the evident scorn felt by the speaker gives the implication. But the speaker may and sometimes does use the imperfect subjunctive to imply contrariety to the fact of the present. The ideal condition may be one existing in the mind of the agent of the main clause. In such a case there will be present some such implication as 'under the pretense that'. But this fact has no bearing on the problem of the tenses. In fact there is no problem of the tenses. The real problem has to do with the origin and development of the construction and particularly with the shift in meaning of the particles and with the question of ellipsis or suppressed apodosis.

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### Houdini and Ἀνάμνησις

The following is from an interview given by the late Harry Houdini to The Detroit Free Press and reprinted in The Literary Digest for November 20, 1926: "I, myself, have entered some Old World city for the first time in my life, so far as I was aware, and found the streets familiar, known just where to go to locate a certain house, for instance. Things have come to me that it seemed could only have been results of some former experiences. I seemed from earliest childhood to have a grasp upon certain faculties and a knowledge not according to my years—as if the understanding were from past education and that I had entered the world with certain fixed principles and ideas that could not have been at that time the result of any present education". With this compare, for example, Plato, *Phaedo* 72 E, or Cicero, *Cato Maior* 78. BROWN UNIVERSITY

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